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A NEO-LAMARCKIAN THEORY OF EVOLUTION.

A Theory of Development and Heredity. By Henry B. Orr, Ph.D., Professor at the Tulane University of Louisiana. (New York and London: Macmillan and Co., 1893.)

THE appearance of a new work on the causes of evolution, by an American biologist, raised the hope that more solid arguments would be forthcoming in favour of the conclusions accepted by so many well-known writers and workers on the other side of the Atlantic. This hope was doomed to disappointment, the evidence adduced being even more slender than usual, and the conclusions even more rash.

According to a view which has been recently put forward by Lamarckian writers, the facts of adaptation are explained by the supposition that organisms possess such a constitution that they are compelled, by the incidence of external forces, to react adaptively, viz. by the production of useful variations. This view appears to be supported by the author (chapter i.), although no evidence is brought forward in favour of it. The hypothesis in question seems to be little more than the old "internal developmental force," or "innate tendency towards perfection," in a modern dress. Furthermore, a little consideration of the essential nature of adaptations proves the futility of any such attempt at explanation. The ultimate objects of adaptations are to obtain food, to escape enemies, or to subserve reproduction. In the vast majority of cases adaptations are relative to the condition of other individuals of the same and other species—a condition which is and has been continually changing. But the environment with which the organism is in continual contact, and which is presumably supposed to bear a pre-eminent part in working these useful variations, is the inorganic environment. So that stimuli provided by one form of environment are looked upon as the direct causes of adaptations which are essentially related to another and very different environment. It would be necessary, in order to make the suggestion valid, to prove that the changes in the organic environment which render some adjustment of adaptation necessary, are invariably accompanied by corresponding changes in those forces (chiefly inorganic) by which it is believed that the adjustment is effected.

In discussing "the limits of natural selection," the following supposition is believed to constitute an insuperable objection to Weismann's theory of heredity, viz. "the supposition that the germ-plasm may exist in the body and still be no more affected by the changes which pass over the body than if it were enclosed in an hermetically sealed vial." It is well known that in all his later writings Weismann has abandoned the hypothesis of a germ-plasm endowed with an almost resistless stability. But the admission of the converse supposition, that the germ-plasm is profoundly affected by the external forces which also affect the body, is very far from the admission that acquired characters are transmitted; and it is this latter, and nothing less, which is required as a foundation for the Lamarckian hypotheses, as, indeed, the author freely admits. In order to prove that the one proposition involves the other, it would be necessary to show that an external force producing a certain effect on the body must produce, upon a totally different thing—the germ-plasm—not, indeed, the same effect, but its precursor.

Considering "the many cases of wholesale destruction of animals—for instance, the killing of countless fishes by a sudden change in the temperature of the ocean, the killing of birds and insects by cold and storms of wind and rain, and the killing of myriads of organisms of all kinds by circumstances over which they have no control, and from which no mere individual variations could save them," the author is led "to doubt that natural selection acts with such mathematical accuracy in accumulating slight individual variations." It is difficult to understand the grounds upon which the non-operation of natural selection in certain kinds of destruction is held to cast doubts upon the efficiency of its action when it does come into operation. Furthermore, it is easy to point to results which are consistent with the view that natural selection may act "with mathematical accuracy" even in the class of cases brought forward. The peculiar condition of the wings of insects living in islands in stormy zones, and a power of resisting temperature related to the needs which follow from the geographical distribution of plants and animals, may be cited as examples.

The author quotes with approval Prof. Cope's contention that "the useful additions which have constituted certain genera, families, orders, &c., what they are," were produced as a direct consequence of needs, and were not formed first and selected afterwards, inasmuch as "it would be incredible that a blind or undirected variation should not fail, in a vast majority of instances, to produce a single case of the beautiful adaptation to means and ends which we see so abundantly around us. The amount of attempt, failure, and consequent destruction would be preposterously large, and in nowise consistent with the facts of teleology as we behold them."

This antithesis suggests that it is believed that "the useful additions" were fully formed as complete organs or parts before they were selected. According to the theory of natural selection, however, such "additions" have been led by selection from the very first—from the time when the parent organ or part first began to be used for a new purpose (for such is almost invariably the origin of the last "addition")—right back to the remote period when the original ancestor of a long succession of organs and parts came into being. But even accepting Prof. Cope's antithesis, surely, when we consider the slow succession of forms throughout geological time, and the amount of extinction which took place in every generation, we may accept his conclusion as to the amount of "attempt, failure, and consequent destruction," and reply that on this very account the Lamarckian explanation is extremely improbable, inasmuch as it would imply a direct and rapid evolution, whereas we know that evolution has been slow and interrupted.

Unnecessary confusion has been introduced into the discussion of instincts by the inclusion under this term of the clearest results of education and experience, containing nothing in common with true instinctive actions. Thus we read on p. 21:—"That the Kea-bird of New Zealand has learned to dig the kidney-fat out of living sheep since the introduction of sheep into that country, is another wonderful instance of change of instinct hardly to be accounted for by means of natural selection, but rather as the result of intelligent experience."

Speaking of instinct, he maintains "this seems to be a well-defined case of the inheritance of acquired characters. We cannot deny that the idea or knowledge has been acquired, neither can we deny that it has been inherited." And yet many instinctive actions of the greatest complexity and interest take place under conditions which preclude the possibility of the acquisition of any idea or knowledge. Thus many insects before pupation take the most elaborate precautions, which tend to prevent their detection by enemies they have never seen and never will see in the helpless stage they are about to enter. If knowledge cannot have had any part in the origin of such marvellous instincts, it is unnecessary to suppose that it is an element in the rise of any truly instinctive action.

In considering the source of organic energy in chapter ii., the author supposes that by the law of conservation of energy all the forces of the environment which act upon an organism must be transferred into some form of energy within it, accounting for "all changes in shape and all changes in the method of activity." In order to help to understand how this may occur, he instances a number of photochemical changes taking place in inorganic bodies. But the law of conservation of energy demands nothing of the kind. The forces which fall upon the organism must indeed persist, but not necessarily in any form which can be made use of by the organism; or they may, and indeed frequently do, serve to expend part of the store of potential energy derived by the organism from other forces. Thus the effect of any external force (*e.g.* light, heat, sound, &c.) upon a peripheral end organ is only conveyed to the brain by the expenditure of potential energy derived from food. The animal may benefit by the information received, but the transmission itself is a loss, and not a gain to the store of force at its disposal.

When, in chapter iii., the author considers in further detail the action of environing forces, many instances are brought forward, in which it is clear that some *necessary condition* of development, such as food or heat, is interpreted as a *cause* of development. It is surprising that the author did not think of applying his conclusion that "food is one of the stimulating forces which guide and determine the developmental reactions of the organism" to the vegetable world; where an endless variety of forms depend upon a food supply which is, when compared with that of animals, remarkably uniform.

The naïve manner in which conclusions are drawn is surprising in an author who is evidently a trained biologist, and is accustomed to the indirect and excessively complex relation of causes to effects met with in organic nature. Thus, on p. 47 we are told that "pigment increases in quantity or brilliancy as we approach the equator, where the light is most intense. There is thus a direct ratio between the amount of light and amount of pigment"; and again on p. 49: "The plain facts lead to the conclusion that pigment is caused by light acting upon the tissues, and where there is no light there can be no pigment." It is interesting to compare these bald statements with the evidence produced by Dr. A. R. Wallace, that there is no proportionate increase in quantity or brilliancy of colour—either animal or vegetable—in tropical as compared with temperate countries; but that such increase as there is, is merely proportionate to the increase in variety and number of forms of life. But apart from this deliberate conviction of one whose opinion on such a subject is entitled to so much consideration, the mode of development of pigment in animals is entirely opposed to the author's conclusions. The bright colours of insect-imagines are developed in an earlier stage, during which they are comparatively screened from light, and when in the final stage the colours are exposed, they undergo no change in the direction of increasing brilliancy. Even such changes as the darkening of the freshly exposed pupal cuticle—long assumed to be photochemical—have been shown by the writer to be independent of light, and almost certainly due to oxidation.

Equally rash and ill-considered is the conclusion, in the same chapter, that the colours of desert animals and of those living among the sea-weed of the Gulf Stream are due to a direct photochemical influence of the respective environments, and that the whiteness of Arctic animals "may be primarily due to the small amount of light in those regions." In reaching this last conclusion the author relies upon the supposed fact that *all* classes of animals are similarly coloured, forgetting that when, as in the raven &c., a white appearance is not needed for attack or defence, it is not attained.

In considering the action of the nervous system, in chapter iv., it is "safely" concluded that "the energy of the forces, acting from without, persists within the living matter as nervous activity and change of nervous condition." It has been already pointed out that such persistence is frequently attended by a loss rather than a gain to the energy at the command of the organism. The author has thought it worth while to translate long passages from Detmer, and does not appear to be aware that the conclusions he quotes have been refuted by Weismann, and are even rejected by botanists who do not agree with the views of the latter.

In chapter v. it is contended that development has been dependent on association and repetition. It is forgotten that many essential actions are performed but once in the life of the individual (*e.g.* cocoon-spinning), or are as perfectly performed the first time as on subsequent occasions (*e.g.* web-spinning of spiders, sucking of mammals). The author freely admits that such a theory of development demands the transmission of acquired characters, and it is therefore remarkable that his discussion in chapter vii. of this question—the foundation of every conclusion he brings forward—should be so brief, and his reasons for accepting such transmission so unconvincing.

In chapter ix. the author attempts to illustrate in the approved Neo-Lamarckian manner the moulding of limbs and the skeleton generally as the result of the forces to which they are exposed during the life of the individual. In the explanation of the remarkable lengthening of the fore-part of the giraffe we meet with a suggestion which is, I believe, new in the history of Lamarckian speculation. In stretching for food it is contended that the animal "must have constantly raised itself off its fore-feet, and as it dropped must have received a shock that made itself felt from the hoofs through the legs and vertical neck to the head. In the hind-legs the shock would not be felt. . . . The principle of increased growth in the direction of the shock resulting from superabundant repair of the momentary compression, explains how the giraffe acquired the phenomenal length of the bones of its fore-leg and neck; and the absence of the shock in the hind-quarters shows why they remained undeveloped and absurdly disproportionate to the rest of the body." (pp. 164–165.) It is unnecessary to take the trouble to refute the details of the various suggestions brought forward by the upholders of the Lamarckian hypothesis: they refute each other. One of them explains a lengthened neck as the result of extension, another as the result of compression; while neither give any approach to a proof that such an effect is likely to result from the antagonistic forces which they respectively invoke. E. B. P.

CELESTIAL PHOTOGRAPHS.

A Selection of Photographs of Stars, Star Clusters and Nebulæ, together with information concerning the Instruments and the Methods employed in the Pursuit of Celestial Photography. By Isaac Roberts, D.Sc., F.R.S., &c. (London: The Universal Press, 1894.)

THIS handsome collection of celestial photographs is a remarkable example of what can be done single-handed in a new line of research. Taking up celestial photography more than ten years ago, Dr. Roberts has devoted himself entirely to it, and has been rewarded with an amount of success that must be to him a source of intense satisfaction.

This success, however, has not been obtained by merely exposing plates in the telescope and watching the images come out in the developing dish. The work that Dr. Roberts set himself to do was planned at the outset in a thorough manner, and has been carried out apparently without regard to either labour or expense; even to the extent of the removal of the observatory near Liverpool, to a better position, selected after a most exhaustive discussion of weather conditions in the south of England.

In the introduction of his book Dr. Roberts tells us that a reflector of twenty inches aperture was the instrument decided upon, in consultation with Sir Howard Grubb, as the most likely to give the best results. An instrument of this size, arranged for exposing the plate without the use of a plane or prism, was made and mounted by Sir Howard Grubb in 1885. With this instrument all the celestial photographs have been taken.

The first arrangements do not appear to have been quite satisfactory, so that the telescope had in great part to be reconstructed; but even then the star images were found, on trial, to be elongated, though this defect seems to have been remedied later on, if one may judge by the remarkable absence of any deformation of image in some of the very long exposure photographs.

The question of the utility of the photographic chart is discussed, and the fact mentioned that Dr. Roberts had already, in 1885, begun a photographic chart of the northern heavens, and made some progress, when the International Congress for the Photographic Chart of the Heavens took up this work. The use of the paper copies of celestial photographs might, it is suggested, be (1) the detection of changes in the structure of nebulæ; (2) the detection, on a large scale, of movements amongst the stars; (3) determinations of variations in stellar magnitudes; (4) relative distribution of stars in space; (5) detection of new stars and disappearance of others.

In cases, however, where measurements to a second of arc are required to be made, then the original negatives must be used for the purpose; and in this we quite agree with Dr. Roberts. It would have been well worth while to picture, in this connection, the instance given by Dr. Roberts of his comparison of a chart of the heavens, made in 1863 by D'Arrest, showing 212 stars, with a photograph of the same region taken in 1890, which showed that considerable changes had taken place among the stars in this small area of the sky (comprised within one degree of declination and half a degree of right ascension) during the interval of twenty-six years between the epochs of the charts.

The relative advantages of refractors and reflectors as photo-instruments, and the requirements and adjustments of a reflector for celestial photography are discussed; and the ideal instrument for photographing the sun, moon, and planets is given as a refractor of six or eight inches aperture and very great focal length; but for the delineation of faint stars and faint nebulosity, Dr. Roberts gives a preference to the silver-on-glass reflector, and he would choose an instrument of twenty-seven inches clear aperture and eleven feet three inches focal length, the mirror being figured, and every part of the mechanism made in the most perfect manner possible, and with a guiding telescope with an objective ten inches in aperture. To anyone contemplating the erection of an instrument for celestial photography, these hints—the results of many years' experience—should be of great value.

The other chapters, on the collimation of the mirror, the essentials of a photo-telescope, method of testing the stability of a photo-instrument, and photographic plates, their exposures and developments, will be read with great interest by all photo-astronomers.

The chapter on the collimation of the mirror gives an account of the method employed to check the guiding telescope, and is perhaps the only point on which something might be said in the way of criticism of Dr. Roberts' method. It is difficult to see why the method described should not be varied so as to be always available, and so dispense with the guiding telescope; a matter of no slight importance in the case of such an instrument as Dr. Roberts has suggested, the ten-inch objective of which would cost as much or more than the mirror. We